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IS CONSUMPTION EVER CONTAGIOUS, OR COMMUNICATED BY ONE PERSON TO ANOTHER IN ANY MANNER?

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THIS question may seem one of very little importance, because we all may be of one opinion, viz., in favor of the negative of the proposition. Yet it is one about which I have been interested, in consequence of some cases that have fallen under my notice during the last ten or fifteen years. These cases I shall relate to the Society; but before doing so, let me refer briefly to the views of the medical profession on this topic, historically considered. In all that relates to medical opinion previous to the present century, I shall depend on the learned work by Dr. Young.* It would seem, from Dr. Young's statements, that all the chief authors who, previously to the last quarter of the last century, treated of the subject, were full believers in the contagiousness of consumption. It is also well known, that public opinion, which is always the reflection of a preceding medical opinion, sustained that view, at least in the southern portions of Europe. The Italians, until a very recent date, have had certain quarantine and other regulations relative to those dying of this disease, as are common now in smallpox cases. Aristotle, according to Dr. Mason Good, states that this belief in the contagiousness of the disease was common among the Greeks. Galen regarded it as unwise to pass even a day with a consumptive. Morton†, 1619, Riverius,‡ 1656, and above all, a century later, the learned and sagacious Morgagni,§ 1761, would not dissect a phthisical patient's

* Practical and Historical Treatise on Consumptive Diseases, deduced from Original Observations and collected from Authors of all Ages. By Thomas Young, M.D., F.R.S., London, 1815.

† Morton. Phthisiologia, London, 1619, p. 185.

‡ Riverius. Opera Medica, Fol. Geneva, 1728, d. 1656, &c.

§ De Causis et Sedibus Morborum, Fol. Venice, 1761, Ex. xix., 53—xlix., 15.

body because of the risk he thought would be run of taking the disease. Dr. Withering,* in 1775, claims that it is certainly infectious.

Subsequently to this long period of almost entire faith in the contagiousness of the disease, we have a period of doubt and of questioning upon the subject for about twenty-five or thirty years.

Dr. Cullen,† in 1777, dares not assert that it is never contagious, especially in warm climates, but he had never seen a case, in which it was decidedly communicated by one individual to another.

The Edinburgh professor evidently doubted the commonly received opinion, but was unwilling to take a bold stand against the idea of contagiousness, as those of the same school did in 1794 (i. e. seventeen years afterwards), who not only opposed the notion, but presented strong cases against it.‡

Dr. T. Reid,§ of London, 1782, allows that, in extreme cases, it may be slightly contagious, and young persons should not sleep in the same room with a phthisical patient.

During the same year, 1782, Bursieri‖ says the Colleges of Physicians of Tuscany had declared their disbelief in its contagiousness; but this did not prevent government from taking precautions against it.

Emale,¶ 1783, thinks it so contagious that it is dangerous to draw milk from the breasts of a woman, as recommended by Chevillard and others, for the cure of consumption, for fear of the disease being given to the nurse.

Vogel,** 1785, is quite convinced of its contagiousness.

Darwin,†† 1793, is a complete believer of its contagiousness, in *persons nearly allied.*

By these quotations—coupled with the fact that I find none, during the same period, decidedly opposed to the proposition—I think I have given some proof—first, of the very general consent to the proposition of the contagious nature of consumption, until the commencement of the last quarter of the last century; second, that from that time until about 1800, there was evidently great indecision. From 1800, with some very few exceptions, Medical Opinion and Public Opinion have been gradually becoming settled in opposition to the theory of contagion. No one will deny that Morgagni showed great weakness in being unwilling to dissect the body of a phthisical patient. In fact, no one would hesitate to regard the disease, as seen now, wholly innocuous to the degree feared by the great pathologist of the last century.

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* Account of the Foxglove. Birmingham, 1785.
† First Lines of the Practice of Physic, Edinb. 1777.
‡ Dr. Young. Supra, p. 360.
§ Essay on Nature and Cure of Phthisis Pulmonalis. Lond. 1782.
‖ J. B. Bursieri. Institutiones Medicinæ Practicæ, 4 v. Venice, 1782. English by Brown. 4 v. Edinb. 1800.
¶ Emale. Journal Médicale, lxiii. 1785.
** S. C. Vogel. Handbuch der Practischen Arzney-Wissenshaft. 1785.
†† T. Beddoes to E. Darwin, on a New Method of treating Consumption.

I may here remark that one wonders at the fears of Morgagni, when he remembers that Fabricius Hildanus describes several examinations of bodies of persons dying of consumption, and this more than a century before Morgagni lived.

The following resumé of the authorities on the subject since 1793 up to the present time, will include my third period, or that of nearly utter denial of the idea of contagion in connection with consumption.

Only two writers of note and of undoubtedly strong minds, and these Americans, can be found to defend the proposition of contagiousness. These two writers are nearly at the two extremities of the period now under consideration, viz.: Benjamin Rush,* 1793, and Drake,† 1854—about a half a century later. Dr. Rush is fully convinced of the contagiousness of the disease, and quotes what I cannot but think rather apocryphal cases sustaining his views. Dr. Drake, without taking the affirmative decidedly, nevertheless evidently leans very strongly towards it; quotes facts, and argues analogically that such a proposition is quite in accordance with the fitness of things.

Meanwhile a certain number, especially in the earlier part of the century, are in doubt. Dr. Heberden,‡ for example, 1802, has not been able to make up a decided opinion upon the subject; but he had seen several cases in which he could not find any more plausible reason than contagion, from the patient having been constantly in attendance on another, and even sleeping in the same room. The very same year, Haygarth§ and Darwin‖ decided against the proposition.

Wilson,¶ 1803, says it is rarely attributable to contagion, but it is wisest to avoid sleeping with a consumptive patient. Little is said by authors upon the subject till 1809; and from that period up to the present time, authorities may, with the exception of the two first (viz., Rush and Drake), be classed either as being in doubt, and, at times, very decided opposition to the idea of contagion; or they ignore the whole subject, and thus virtually seem to consider it beneath their notice.

In the first class we find Portal,** 1809; Laennec,†† 1818; Cowan,‡‡ 1835; Davies,§§ 1835; Ancel,‖‖ 1852; Watson¶¶, 1857; and

* Medical Inquiries, 1793.
† Diseases of the Interior Valley of North America. 1854, Vol. 2.
‡ Commentarii de Morborum historia et curatione. Lond. 1802.
§ Haygarth. Young, p. 395.
‖ T. Beddoes. Hygeia, or Essays Moral and Physical. Bristol, 1802.
¶ A. P. Wilson. Treatise on Febrile Diseases, 1803.
** Portal. Observations sur la Nature et le Traitment de la Pleuro-Pneumonie. Paris, 1808.
†† Laennec. L'Auscultation Médiate. Paris, 1818.
‡‡ Cowan. Translation of Louis on Phthisis. London, 1835.
§§ T. Davies. Lectures on Diseases of the Lungs and Heart. Lond. 1835.
‖‖ H. Ancel. Treatise on Tuberculosis, the Constitutional Origin of Consumption and Scrofula. Lond. 1852.
¶¶ Thomas Watson. Lectures on the Principles and Practice of Physic. London, 1857.

Cotton,* 1858. All these writers actually oppose the notion. Dr. Watson rather dogmatically would settle the question by declaring that "a *diathesis* cannot be contagious."

In the second class are found the following well-known authors, most of whom have written special treatises on tuberculosis, and some of them have gone quite elaborately into the causes of consumption. Louis,† 1825; C. J. B. Williams,‡ 1834; Stokes,§ 1843; Blakiston,‖ 1848; Copland;¶ Bennett,** 1858; Lawson,†† 1861; Todd,‡‡ 1861.

From these quotations it is evident that, at the present time, we may say that Medical and Public Opinion are almost the exact reverse of what they were previously to the middle of the last century. My own doubts have disappeared under the close examination of my own cases, and of those reported by others. Nevertheless, I should not agree with the extremes of either party, but should hold, that while there is no positive proof of the real contagiousness of consumption, it would be very unwise to deny that some cases do arise, in which a close attendance upon and devotion to persons affected with phthisis is the prominent exciting cause of tuberculous disease of the lungs, in persons apparently healthy.

Similar views are held by others. In proof, I quote as follows: John Hogg,§§ 1860, remarks it is difficult to say if contagion can be fairly classed among the causes of consumption; but, he adds, it would be the *acmé* of imprudence to place a young person, not perhaps in the best of health herself, in close attendance on or near another. To this last expression of opinion I should give my adhesion, but I would not limit the remark to those who are not in good health—for I think our cases bear out the assertion that it is hazardous for even the most healthy to run that risk. A similar opinion is also held by Mons. Gueneau de Mussy,‖‖ clinical successor to Chomel, in Paris. He declares, 1864, wholly against contagion, as usually understood, but at the same time believes that it is probably transmitted by coition and by very intimate relations with an individual during any of the periods of the disease. At the time of writing he had two cases in the wards of Hotel Dieu. He quotes Andral as coinciding with him. He has observed a trouble in the throat among

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* R. Payne Cotton. On Consumption, its Nature, &c. London, 1858.
† P. C. H. Louis. Sur la Phthisie. Paris, 1825.
‡ C. J. B. Williams. Physical Signs of Diseases of the Chest. Am. Ed., 1834.
§ W. Stokes. Diseases of the Chest. 2d Am. Ed., 1844.
‖ G. Peyton Blakiston. Practical Observations on Diseases of the Chest. London. Am. Edition, 1848.
¶ Medical Dictionary. Article, Tubercular Consumption.
** J. H. Bennett. Clinical Lectures. American Edition. New York, 1858.
†† L. M. Lawson. Practical Treatise on Pulmonary Consumption. Cincinnati, 1861.
‡‡ R. B. Todd. Clinical Lectures. London, 1861.
§§ John Hogg. Practical Observations on the Prevention of Consumption. London, 1860.
‖‖ Leçons sur les causes et le traitement de la tuberculose pulmonaire. Hotel Dieu, Union Medicale, vol. 4, p. 374, Nov., 1859.

those who live with the tuberculous. He thinks it more common for consumption to be transmitted from a man to a woman than from a woman to a man.

These general statements give a glance, at least, at the three periods of opinion, and the commencement of a fourth, on this subject; viz., 1st, the period of almost undoubting faith—extending from the earliest records of medicine up to 1775, or thereabout; 2d, a short period, of 25 or 30 years, of great and painful doubt; 3d, a time of utter scepticism; and, 4th, perhaps I may add, a modified faith, not coinciding with either the "everlasting Yea," "the centre of indifference," or "everlasting Nay," as Carlyle would express it.

Let me proceed now to the examination of my own cases.

CASE I.—A gentleman died Feb. 13, 1858, after two years' illness, with confirmed and undoubted phthisis. I saw him and examined him, and know that he had all the marked rational and physical phenomena of the disease.

The wife consulted me March 4, 1864, with decided signs of phthisis. It appears that her family was usually quite healthy. Parents alive: mother 56 years old, asthmatic; father well. One uncle, by the father's side, had died of consumption. She was an only child, and had always been, up to the time of her husband's illness, in most robust health. In fact, she never "dreamed of being ill," either generally or locally. She was her husband's sole nurse during his year's illness. She slept with him to the last. He complained if she left him. He wanted the warmth of her body to keep off his chills at night. Often her clothing was wet by the moisture from his body. Her parents protested against this course, and warned her of the consequences; but the wish expressed by the husband, her own confidence in her own health, and abandonment of all thought of self for her husband's sake; these considerations overcame all others, and she continued this course until the end of his life. When this time arrived, she found herself wholly worn down, and she had never been well afterwards. She had been much less capable of endurance, and was easily fatigued by comparatively light work. Every spring she had been worse than at other portions of the year. She had had severe cough from the first, with aggravations of it at times, but with only slight sputa. A year since, hæmoptysis occurred—about ʒ i., bright, arterial. She had had more or less pain about the right shoulder; had been able to lie on either side, and had never been seriously prostrated. Her hæmoptysis occurred after the performance of a very difficult piece of music, requiring extra physical exertion and mental attention. She had had at times some dyspnœa; no palpitation, but was easily tired by walking. Her appetite had been fair, with good digestion as its attendant. Menses regular—urine variable, at times very light colored. She had had a series of periods of emaciation, and subsequently some gaining of flesh, but with a gradual loss of 25 pounds—having fallen from 150

to 125, her present weight. Chills and heat, but no sweating had occurred. This spring, for the first time, she had had hoarseness, and coughed, morning and evening, in paroxysms of about half an hour. She had taken iron and other tonics with benefit, and whiskey not regularly. When I saw her, her countenance was a little pale, not much emaciated, or showing the marks of severe disease. Pulse 88. Tongue clean. The physical signs were as follows:—There was flatness behind right clavicle, and dry crackling to the fourth rib. Voice a little resonant. Flatness also at the top of the right back, and a coarse crackle and slight resonance of voice. Rales, though less marked, even to the base of the lung, and respiratory murmur rather rough even there. Respiration in left lung noisy, somewhat sibilous.

She evidently had tubercles, slowly developing, at the top of the right lung. She considered that this was the culmination of a disease excited by her close attention to her husband during his illness. The facts of the mother's "asthma," and the uncle's death by phthisis, may throw some doubt as to the contact with her husband being the chief cause of the disease in her case. She was hereditarily (so the sceptics will say) tending to phthisical disease—and the over-exertion, &c., broke her down, and made her an easy victim.

I might, in reply, say, that if we take the simple fact of one uncle having died of phthisis as a proof of the *hereditary* disposition, and this in opposition to a most perfectly robust health up to the time of beginning to attend her husband, I cannot but think as much may be said of almost every family. There is scarcely one in this community in which some member, remote it may be, has not had symptoms of phthisis. The "*asthma*" of the mother does not really bear upon the subject of *her phthisis*.

Case II.—Miss E., a young and very healthy-looking woman, I saw in November, 1855. She was the sole attendant of a very sick, tuberculous husband, and slept with him during all the periods of his disease. I was struck with her apparently robust health. A short time after her husband's death she had hæmoptysis. She is now, I believe, alive and married again, but is ill with phthisis, which commenced then, but which makes, however, very slow progress, and does not seem to very *materially* impair her health.

Case III.—In 1855 I attended Mr. H., who was an invalid with pleurisy, and subsequently tubercular disease appeared. He remained for years, fighting against the complaint, and finally died with tubercular pneumothorax. His wife's mother died of phthisis, but she herself was splendid in her kind of robust English form, when her husband fell ill. She *devoted* herself without stint to him, and gradually began to "heck." On examination, physical signs were noticed under the clavicle. The symptoms rapidly increased, and she died before her husband, and was evidently tuberculous.

Those knowing the facts believe, as I believe, that if she had not attended upon him so closely and so long, she might be now alive.

Case IV.—Miss S. sprung from a family, in which no trace of consumption is known ever to have existed. She was living as a farmer's daughter on a most healthy site, and enjoyed most strong and robust health, when she went to attend an invalid friend, to whom she was most tenderly attached. The friendship was mutual. The invalid sought for and received the closest attention on the part of her young companion. They slept in adjacent rooms at night, and the strong one acted as nurse at all hours, day and night. This attendance commenced in July, 1853, and continued till June, 1854, eleven months, when, at the request of her parents, our patient went home. In August, however, she again spent a week with her friend, and was with her at her death, August 30, 1854. During all this period the invalid did not wish any one else to do aught for her, and her young companion often lifted her when the patient was too feeble to raise herself. During one of these efforts Miss S. felt that she had "strained herself." Ever after she had an uncomfortable "coldness" at the epigastrium and a tendency to chilliness—with, subsequently, in the spring of 1855, some dyspepsia. In the following months, her parents noticed her rather feeble condition, but did not regard her as seriously ill. She was treated as a dyspeptic, and considered herself as such. She had scarlet fever in the spring. In August, 1855, cough set in, i. e. after twelve months' invalidism, and the cough continued uninterruptedly till death. Diarrhœa commenced in March, 1856. She died of phthisis, July 27th, 1856, nearly two years after her friend's decease. I saw her once in the spring of 1856. She then had the signs of anomalous tuberculosis, i. e. crepitus, a pure tubular respiration, and solidification of the lower two thirds of the right lung. She also had had two abscesses under the arms, a few days before my visit.

Case V.—A gentleman whom I was attending in advanced phthisis, in 1855, assured me that, though one brother had died of phthisis, after some particular exposure, he did not deem himself hereditarily disposed to the disease, as no other case had occurred in the family. This gentleman attended constantly on his wife, who died of phthisis. On my asking whether he thought that attendance had had any influence on his own health, he remarked instantly, as if the idea had previously occurred to him, that the *first* marked paroxysm of coughing he had ever had, and from which he dated all his own ills, was when he was raising his wife in bed and inhaled very deeply her breath. It caused much irritation at the time, and coughing was produced, which continued afterwards.

Case VI.—Mrs. ——— I saw, October, 1854, aged 30 years. She had been taken suddenly with hæmoptysis. She had had five attacks of the same previously—the first at 18 years of age. Usually they had been of slight duration, and in the interval her

health had been excellent. She had slight disease at the top of both lungs. She had been the sole attendant of her husband, who had died about two years previously of phthisis. She had often risen in the night and given him her warm and dry night dress, and moved him to her side of the bed, while she lay down upon the mattress, wet with his sweat, and with only a blanket thrown over it. This was only one of the numerous acts of self-sacrifice which she performed. She told me she knew she was running a risk, but she did not, at that time, care for it. From the time of her husband's death her own health had been materially changed—she had never been as well as before. After contending against the disease ten years, and being at times able to labor much, as superintendent of a large mantilla establishment, she is now in a hopeless state of disease. But she is fully convinced that the close attention to her husband first undermined her health.

Case VII.—Mrs. ———. This is doubtful as to present diagnosis—but I fear phthisis will result. She has no hereditary tendency, and is a stout, full built woman, and was, till lately, healthy. She attended her husband, who died of phthisis in June last, 1863, after nine months' illness. She was his sole nurse. She slept with him till the last, and scarcely left his room for three or four months preceding his death. She was, of course, fatigued, but went into the country and felt sufficiently well, except she had a liability to headache, to which she was not subject before. In November or December, 1863, she first noticed some dyspnœa; afterwards some palpitation and some sweating at night. These symptoms increased, and she was thought to have phthisis. I found evidence of latent pleuritic effusion filling the whole of the left chest. Tapping was performed Feb. 9, 1864, with relief to the dyspnœa. Between three and four pints of serous fluid was drawn out, but there has been no frank improvement of the local signs. The lung has never freely expanded, but, on the contrary, dulness has always remained, and only the finest crepitus or cough is produced at upper part of lung.

May 14, 1864.—Since this was written, the lung has expanded more, and the fluid that remained has been absorbed. There is, however, still an obscurity of murmur and much less resonance than usual.

Case VIII.—I saw two sisters in the central part of Massachusetts. I visited them in October, 1863. The parents cough. The grandmother, by the mother's side, had two aunts and a sister die of consumption. Evidently the disease strongly tends to occur in the family. One of the present sisters was dying of advanced tuberculous disease; she had had slight cough for two years, but only broke down two months before I saw her, and apparently from overexposure, at a celebration on the return of soldiers to her native town, she being chief manager. The whole of left lung had the fine crepitus of recent tubercles, and there were signs of a few at the top of the right. The sister was devoted to her, and declared she would

not leave her. She watched with her, and would not take even the slightest precaution. I warned her of danger, but she paid no heed, careless of everything save attention to the sick one.

The physician has written to me, within the past six weeks, that she, too, is beginning to show signs of phthisis, and is, in reality, more rapidly sinking than her sister.

CONCLUSIONS, FROM THESE CASES.

Out of the 8, we have 7 females, viz., 5 wives, 1 sister, 1 female friend; and only 1 male.

They may be divided into three classes, according to their hereditary tendency to phthisis.

1st. Possibly, phthisical tendencies previous to exposure, though apparently healthy at commencement of attendance, } 5

2d. No such tendencies, 2

3d. Doubtful, but appeared healthy, 1

In 1st class, only one had had symptoms previously, viz., hæmoptysis, years previously, without serious influence on the health. In the four others, one or more members of the family had had phthisis; viz., one mother, one uncle, one brother, and one sister.

The opponents of the idea that contagion had anything to do with the production of phthisis in the 1st and also in the 3d class, viz., in six of the eight cases given, will argue that these cases are utterly worthless as a ground-work of any argument for contagion, and therefore they should be put wholly out of the question. I cannot entirely agree to this view of the subject, when I remember that, without exception, my patients were all, apparently, in good health before they fell under the supposed deleterious influence of close attendance upon the consumptive friend; and that from the termination of that attendance the health of all was found to be undermined. Hence I really think I can claim, with a certain degree of reason, that, whatever were the hereditary tendencies, the attentions in nursing the phthisical friends were the *exciting* cause of their own disease.

But let us examine the two remaining cases:—And here also we must admit a certain lameness in one of them as a basis of argument, viz., a doubt about the *certainty* of the diagnosis. So we are really reduced to only one perfectly reliable case, in which no trace of tuberculosis can be found previously to the exposure. Now in this case, if we can ever allow ourselves to speak of causes in diseases, the phthisis and subsequent death were not only a sequence, but I think and others think a veritable consequence, in the strictest relation of cause and effect, of her previous close attendance on a tuberculous patient. But was there necessarily contagion in this case? By no means, the anti-contagionists will reply. It may well be questioned whether anything at all similar to contagion, as we

usually understand that term, can be predicated of it. There is proof enough of weakened health, of dyspepsia, and its usual concomitants, and after months of gradually deteriorating health phthisis did set in. But then the same result might have occurred without any attendance on any body. It is the most common way for phthisis to develope itself. Confinement in any bad atmosphere, ill ventilated apartments, anxiety and depression of mind, irregular digestion, have often produced identically the same results. Then, again, they may say—If contagion had anything really to do with it, why did it prove so long in showing itself? Usually contagion shows itself soon. This was months in developing itself.

Dr. Drake meets this argument, by saying that we really know nothing of the length of time necessary for the incubation of phthisical influences. One disease is promptly contagious, and runs its course rapidly. This is no reason that the same rapid course should be pursued in phthisis. On the contrary, all *its* phases are slow. Why not its period of incubation? In fact, have we not a proof of this in the years that pass over before an hereditary tendency bursts forth? The arguments against contagion drawn from the long period elapsing between exposure and the evident disease is therefore really of small moment. But it falls wholly, when we remember how hydrophobia and other deleterious influences may remain dormant for an indefinite length of time—how miasmatic influences may remain years unknown, and then suddenly show themselves.

But again, say the anti-contagionists, the nature of consumption is very different, certainly, from the majority of contagious diseases; there is no eruption as in smallpox, measles, scabies, syphilis, &c. Above all, the actual inoculations by Kostum, Le Pelletier, &c., proved abortive.

To this the contagionists might reply—But there is whooping-cough, mumps, some sore throats, pleuro-pneumonia in animals, &c., from which there is not apparently any morbific matter recognizable by the senses. Yet these are capable of producing their kind. In regard to actual experiments of inoculation, it is asserted that they were not satisfactorily performed.

Still further, the non-contagionists say—If contagion acts, why have we not had more cases in these many years, and why such a vast preponderance of married females amongst these patients? Does contagion usually select women? Why not rather suppose that the entire *abandon* with which women throw themselves into the duty of nursing; their total recklessness, at times, when attending on a dear friend, of the commonest rules of hygiene in regard to air, exercise, food and sleep—that these, conjoined with and acting upon an excessively sensitive nervous nature, are the causes of the disease in those cases?

We may thus argue pro and con upon the subject, and yet not arrive at exact and definite opinions, as to the precise matter in dis-

pute; and I will close this part of my paper with the single remark, that the strongest argument I know against contagion, is the fact that, considering my facts at least, woman is more liable than man to be taken. Why this, if there be not something at any rate more important than contagion to explain the phenomena of the case? The really contagious disease spares neither age nor sex. There must be something, therefore, more than mere contagion.

FINAL STATEMENT OF VIEWS.

Let me say that I commenced this investigation strongly prepossessed with the idea (gained while attending my patients and with a cursory knowledge of their antecedents) of the essentially contagious nature of phthisis, under certain circumstances. The force of my own facts, when thoroughly examined, has led me materially to doubt the correctness of my former impressions.

I will lay down the following propositions, which I believe correct.

First.—Consumption is not *contagious* in the usual acceptation of that word.

Second.—It may be *infectious*, and to this extent only. By long attendance of the closest kind, by inhaling the breath of the phthisical patient, by living in the phthisical atmosphere, so to speak, and in general by a neglect of hygienic laws during such attendance, the health may be undermined and phthisis set in. How far the depressing passions may have an effect, it may be impossible to say; but that they exert an important and deleterious influence, we cannot deny. Doubtless the want of exercise and the neglect of all hygienic laws have an immense influence. But as we see all these influences, except the supposed infection, existing often, for a long time, without producing phthisis (for example, a long attendance on patients ill with other non-tubercular diseases), I do believe that, in some few instances at least, we must admit a degree of real infection in our estimate of the matter. Especially must we take this into consideration in the case of a sister or a brother in a family, where the hereditary tendency to phthisis is strong; for in this case I doubt whether it would be safe to allow the sister to attend as nurse, even with the best moral and physical hygienic influences.

I am confirmed in this view by two more considerations. We see carcinoma and other chronic diseases have never even suggested this idea of contagion to the medical profession, at least so strongly as phthisis has. Is not this an argument in favor of the idea of there being something peculiar in tubercle, whereby it is capable, at certain periods of its development, of producing its kind? Second, I deem of some importance the positive statement made to me by a husband, the sole male patient among our cases. He distinctly asserted that the first real cough arose when he was raising his wife in bed, and consequently was making an effort and drew in a deep breath, inhaling at the moment while his wife was exhaling. The

chief part of the air he drew in was loaded with emanations from her lungs. As she was, at that time, in the last stages of phthisis, those lungs were probably ulcerated, and possibly some irritating matters may have escaped with the expired air, and have been inhaled by the husband.

PRACTICAL CONSIDERATIONS, ARISING FROM THESE CASES.

While we may feel assured that contagion, as held by Morgagni and the Italians, is, at least in this country, a delusion—we may feel, I think, equally assured that we should warn a wife or a sister or near female friend from devoting herself too closely to the attendance upon a consumptive husband, sister or friend. For the sake of the attendant as well as the sick one, sleeping in the same bed, or even in the same room, should be avoided. If, however, this cannot be prevented, then we should try to eliminate all deleterious influences as much as possible. The room should be thoroughly ventilated and cleansed daily. The attendant should eat regularly and pay especial attention to the digestive system. The diet should be nourishing and simple. It would not be amiss for her to take some wine daily. She should walk daily out of doors, and not confine herself to the sick chamber week after week without release. In actual attendance she should beware of inhaling the breath of the invalid. Disinfecting agents might, with advantage, be used in the spittoons. In a word, every good hygienic influence should be brought to bear upon the attendant. At the same time I would have the community understand that, with such care, there is scarcely a trace of danger in any case, save that of a sister, or a wife whose husband is so miserably exacting or thoughtless as to demand that she should sleep with him during his illness. The danger of the health being injured in these latter cases is great, even if the above hygienic rules be strictly adhered to. The best plan is not to allow a sister or a wife ever to take the *sole* charge of the invalid. Whenever it is possible, let a regular hired attendant assume all the harder work, and sleep in the room, if need be, at night, or, still better, in an adjacent room. The wife or sister should be in the room only during the day, and even then with proper and regular intervals of removal to the fresh air. These rules it may often be impossible to carry out; still oftener, they *will* not be. Nevertheless, I believe our cases fully show that they are simply rational and right, and rules that the contagionist and anti-contagionist ought alike to adopt.

NEURALGIC AFFECTIONS AFTER INJURIES OF NERVES.

[Communicated for the Boston Medical and Surgical Journal.]

MESSRS. EDITORS,—Having read with interest the article entitled "Neuralgic affections after injuries of nerves," by J. Mason War-

ren, M.D., in the last number of *The American Journal of the Medical Sciences*, I have been induced to send you the history of the following case which came under my care while in the army.

A private in one of the Massachusetts regiments received, at the battle of "Chantilly," a wound caused by the passage of a Minié ball through the fleshy part of the thigh, posterior to the junction of the upper and middle third. The wound had been treated in the usual way, with water dressings and rest, and had quite healed when I was ordered to the Lyceum Hospital, in Alexandria, Va., where the patient was, about six weeks after the receipt of the injury. The patient soon after began to complain of pain in the sole of his foot and the calf of his leg, with nocturnal exacerbations, severe and of a shooting character. Whenever the limb was moved, the suffering was increased. I administered opium and the other anodynes in turn, freely, with but little good result. The only night's sleep which I was able to procure for him, followed the endermic application of sulphate of morphia to a blistered surface over the apex of the sacral plexus, and even this failed on a second application. Thus, after the wound was healed, the patient was in a worse condition than before, being deprived both of sleep and the slightest use of his limb.

Several similar cases having been benefited by the exhibition of strychnia, in the neighboring hospitals, I determined to try it in this case. I commenced with 1-16th of a grain, three times a day. The first night, after three doses had been taken, the patient slept well. At 4, P.M., of the second day, when I made my afternoon visit, he was more comfortable than he had been for some time, and felt quite hopeful of recovery, but at about 8, P.M., the time when his nocturnal pains had usually commenced, I was called by the nurse who told me the patient had a fit. I found him in a state of *strychnia spasm*, and yelling with the intense pain which he suffered during the paroxysms. The extension of the lower extremities, especially of the injured one, was very powerful indeed; the legs were shot out with such force as to shake the floor of the building, and the rigidity of the muscles was very marked. I administered 1 1-2 grains of sulphate of morphia, in divided doses, during the first half-hour, with but little appreciable result, and then gave, by inhalation, a mixture of ether and chloroform. Complete anæsthesia was not induced, but the spasms were relieved, after a considerable quantity of the anæsthetic had been administered, and any tendency to their recurrence was met by repeated applications of the sponge, during about two hours, when the patient fell asleep. The next morning he awoke entirely free from the neuralgic pain, and did not have a trace of it during the subsequent time he remained under my charge—about four weeks. His leg, however, was exceedingly sore for some days, as if it had been bruised. The patient was of a nervous temperament.

The explanation which I had made to myself of this case was,

that the great sciatic nerve having been implicated in the cicatrix of the wound, thus causing the increase of pain upon moving the limb, was set free, and the adhesions were broken up by the violent muscular action. This explanation seems, to me, justified by the conditions which Dr. Warren speaks of as occurring in the healing of wounds in the vicinity of large nerves.

Very respectfully, DANIEL D. GILBERT,
U. S. Str. "Maratanza," } *Ass't Surgeon U. S. N.*
May 10th, 1864. }

RARITY OF PHTHISIS AT HIGH ELEVATIONS.

[WE take the following extract from an interesting article on the Climate of the Swiss Alps, by Dr. Hermann Weber, published in the *Dublin Quarterly Journal*. It has an important bearing on the question of selection of residence for phthisical patients in this country.—EDS.]

Tubercular phthisis occurs not rarely in the lower mountainous or sub-Alpine region, but in the true Alpine region it seems to be almost absent. Thus it is of very rare occurrence among the priests on the Great St. Bernard; and Dr. Brügger has scarcely ever observed it amongst those inhabitants of the Upper Engadin who have not resided in other countries, and has further found that this disease is generally cured, in natives of the Engadin, when they return to their mountains, before it has made great progress. Dr. Albert, of Briançon, in the Dauphiné (4,283 feet above the sea level), bears, according to Lombard (l. c., p. 93), the same testimony. These observations are quite in harmony with what we know of the occurrence of tubercular phthisis in other mountainous countries. Thus patients affected with phthisis at Lima are sent on the adjacent mountains of Peru, where phthisis is scarcely known at an elevation of about 8,000 feet. It is described as very rare at Mexico (7,000 feet) and Quito (8,700 feet), and still more so in higher elevations. The elevation, beyond which phthisis becomes rare or is absent, seems to vary considerably in different latitudes, and to become lower as we proceed toward the Poles. In the tropical zone it may be regarded as becoming rare above 7,000 feet; in the warmer temperate zone, above 3,500 to 5,000 feet; in the colder temperate zone, above 1,300 to 3,000 feet elevation. In Switzerland, between 46° and 48° N. latitude, the frequency of its occurrence diminishes above 3,000 feet; in the Black Forest, between 47° and 49° N. latitude, above 2,500; in the mountains of Thüringen and Silesia, and in the Harz, between 50° and 52° N. latitude, above 1,200 to 1,400 feet. Fuchs (*Medicinishe Geographie*, 1853, p. 35) states that at Brotterode (1,840 feet), in the Mountains of Thüringen, the percentage of deaths from phthisis is only 0·9. Brehmer assures us that in the

neighborhood of Görbersdorf, in Silesia (1,700 feet), tubercular phthisis has never been seen by him amongst the inhabitants—(*Die Chronische Lungen-schwindsucht*, Berlin, 1857, p. 134)—an observation which Dr. H. Beigel, who has for several years resided at Reinerz (1,700 feet above the level of the sea, and very near to Görbersdorf), has, in a personal communication to me, to a great degree confirmed.

Scrofulous affections, in all their varieties, are not rarely met with in the Alps, especially in the narrow valleys. The insufficient food, the want of sunlight in the deep valleys during the greater part of the day, the crowded and ill-ventilated condition of the dwellings, to which the inhabitants are confined during by far the greatest part of the cold season, and which they share with their beasts, offer ample reasons for the explanation of this circumstance, which certainly ought not to be attributed to mountain air—the less so as in the true Alpine regions these affections are almost absent, or existing only in mild forms, as testified by Dr. Brügger for the Upper Engadin, and by several other medical men practising in the higher Alps (Lombard, l. c., p. 97).

THE BOSTON MEDICAL AND SURGICAL JOURNAL.

BOSTON: THURDSAY, MAY 26, 1864.

The New Hospital.—By the invitation of the Trustees and Medical Staff, the physicians of the city and vicinity had an opportunity on Saturday last of visiting the City Hospital of Boston before its public opening. A large number of gentlemen met to satisfy a curiosity as to the form in which this long-considered, vexed and much-desired project had at last been realized. They were received by Alderman Norcross and others of the Trustees, and were conducted over the establishment by the Superintendent. We think that not one of those who were then there for the first time had any previous conception of what modern skill in internal architectural appliances and apparently unlimited resources might accomplish in the construction of a hospital. To those who had spent a portion of their student life in European mediæval hospitals, such as the vast lazarettos of Italy, once filled with plague-stricken inmates, or the monastic and sombre institutions of Germany, or the old halls of Hotel Dieu of Paris, all simple structures of plainest stone and wood and plaster, but with steps worn by feet of many generations of students and famous teachers of our art, it seemed hardly possible that all this magnificence was also a hospital, unless designed for some exalted class of patients. Certainly there is none like it in the world, was a frequent exclamation. And yet with all the splendor of marble floors, rich carpets, electric clocks, lofty dome and corridors and pavilions, and the beautiful and grand systems of machinery for heating and ventilating the building, for washing and wringing and ironing the linen, and for the comfort of

all its inmates, which it may gratify the pride of a wealthy city to have founded, it is to be remembered that its success as an institution after all rests almost wholly with the members of our profession, to whom it is so soon to be given in charge, and it is to them that the public will look to see that the large amount of money thus expended is properly devoted to its divinest purpose.

In a future number we hope to be able to give our readers a full description of the internal arrangements and novelties in construction of a hospital, which in all points but size must rank among the most perfect yet built. In some of its details it is indeed open to criticism, but these defects are such as can be easily remedied. Two wings are now completed and furnished for the reception of patients, affording present accommodation for one hundred and fifty patients, medical and surgical, in addition to the elegant private wards in the central building. It is to be open to the sick upon the first of June. We are sorry to hear, however, that the Trustees have decided to exclude from its walls just the class of patients for which alone there was any imperative need of hospital accommodation, namely, lying-in women, and those affected with venereal and contagious diseases. It seems to be the design to make it another Massachusetts General Hospital, a plan which could not be too highly praised were two institutions of that character needed at present in this city: but it appears to us that, if this exclusive system is allowed to prevail, the petition of the physicians of Boston has not been answered, and that we still have as great need of a *city* hospital as before.

The public dedication of the building took place on Tuesday morning, 24th inst., the exercises being conducted in one of the upper wards of the north pavilion. The hall was filled with ladies and gentlemen, and the addresses were of a very interesting character. The profession is particularly indebted to Ex-Alderman Amory for his devotion to their interests in connection with this institution from its beginning, and for the generous appreciation of their services expressed in his excellent address.

ORDER OF EXERCISES.

I.—Introductory by the Choir.
II.—Delivery of the Keys by the Committee on Public Buildings to His Honor the Mayor.
III.—Delivery of the Keys by His Honor the Mayor to the Board of Trustees.
IV.—Reply of Alderman Norcross, Chairman of the Board of Trustees.
V.—Prayer.
VI.—Address by Thomas C. Amory, Jr., Esq.
VII.—Hymn.
Benediction.

Board of Trustees of the City Hospital.—Otis Norcross, George W. Warren, David H. Coolidge, Wm. Cumston, John T. Bradlee, Dr. Wm. R. Lawrence, Theodore Metcalf, Sumner Crosby.

WHAT IS AN ORGANISM?—It used to be taught, and until recently was very generally believed, by the highest scientific authorities, as well as by unlearned persons, that certain phenomena occurring in living beings were in their very nature essentially distinct from any changes taking place, or which could be caused by man to take place, in inorganic matter. And, as a consequence, until very recently, *living* organisms have been regarded as in some way quite different from

things inanimate. *Vital actions* were supposed to be different from *physical and chemical actions.*

But it has long been known that physical and chemical changes take place in living organisms, and of late it has been stated that these physical and chemical actions are the *only actions* which occur, and hence they are the "vital actions" which were believed in by a school gone by. If this be so, it would be correct to use the words "organism," "vital," &c., in cases in which it was not possible that ordinary life could be sustained, or it would be correct to give up the use of these words altogether. Inorganic matter in a certain state, but, as far as we know from experiment, utterly incompatible with life, has been said to exhibit *vital* phenomena, although it cannot *live* according to the ordinary sense of the word. On the other hand, things which undoubtedly do live, are said to exhibit changes resulting from the action of ordinary force. "Organizing force" is but another mode of heat.

If the development of heat, light and electricity results from vital action, and be characteristic of organisms, why should not a mass of inorganic matter which exhibits heat, light and electricity be an organism? But it has not been shown that heat, light and electricity result from *vital action*; and, until this has been done, it must be more correct to regard them as *physical phenomena* wherever they occur, than as vital phenomena when they are manifested in inorganic matter.

But, if it were true that there is no real distinction between *vital and physical and chemical actions*—if there were, indeed, *no actions* occurring in living organisms peculiar to living organisms alone—let the use of all terms with which mysterious (vital) agencies are associated be abandoned; for, by retaining them, it is clear that we retard the real progress of science, and inculcate ideas which are not supported by facts, and which are really destitute of truth.

But it seems to me that many high authorities have rejected the idea of the existence of peculiar and unexplained actions (vital) in living beings, not upon insufficient evidence, but without any evidence to justify such a step; and it is not a little remarkable that not one of those who have accepted and taught these doctrines has ventured to describe what, according to his notion, takes place in living matter. It is right that these questions should be fairly investigated, and they are questions well worthy of attentive study, if the only object be to assign to many words now in common use, and employed very vaguely and in different senses, a definite and precise meaning.

Sir John Herschel has more than hinted that the term "organism" may be applied to matter even in a state of incandescence. Speaking of the willow-leaf like bodies discovered by Mr. Nasmyth upon the surface of the photosphere of the sun, he says:—These flakes, be they what they may, "are evidently the immediate sources of the solar light and heat by whatever mechanism or whatever processes they may be enabled to *develope*, and, as it were, *elaborate these elements from the bosom of the non-luminous fluid in which they appear to float.* Looked at in this point of view, we cannot refuse to regard them as *organisms of some peculiar and amazing kind*; and, though it would be too daring to speak of such *organization* as partaking of the nature of life, *yet we do know that vital action is competent to develop both heat, light and electricity.*"—(*Good Words*, 1863, p. 282.)

But the President of the British Association goes a step further, and even ventures to suggest the particular class of organisms with which these bodies may be compared. He thinks they are shaped like *diatoms!* "I have still to advert to Mr. Nasmyth's remarkable discovery that the bright surface of the sun is composed of an aggregation of apparently solid forms, shaped like willow-leaves, or some *well-known* forms of Diatomaceæ, and interlacing with one another in every direction. The forms are so regular in size and shape as to have led to a suggestion from one of our profoundest philosophers of their being *organisms, possibly even partaking of the nature of life*, but, at all events, closely connected with the *heating* and *vivifying* influences of the sun." (Report of Sir William Armstrong's Address to the British Association, *Athenæum*, August 29th, 1863.) In the above quotations the italics are my own.

The word "organism" is here applied to sheets, flakes, or scales having some sort of solidity, about 1000 miles in length, and 200 or 300 in breadth, the temperature of which is far higher than would be sufficient to keep the most refractory metals in a state of complete fusion, if not to convert them into vapor. With true respect, but with the utmost earnestness, I venture to express the opinion that "organism" cannot be properly applied to these bodies.

I will advance my objections *seriatim*, and I hope to be able to show that there are actions going on in every kind of living matter totally different in their nature to any actions which are known to occur in ordinary inorganic matter. I submit, in spite of many assertions, but *mere assertions* to the contrary, that we must still draw a most definite and well-marked distinction between mere physical and chemical changes, whether they occur in things that "live" or in inanimate inorganic matter: and *those peculiar "vital actions"* which alone occur in matter that is alive. In these papers it will be my object to attach a precise and definite meaning to the terms "vital," "life," &c., and I shall endeavor to state the arguments in favor of this view as simply and clearly as possible. The evidence which I shall bring forward, I think, justifies the inference that these solar bodies regarded as "organisms," differ from any known organisms in so many essential particulars as to render this term altogether inappropriate. I hold that this term "organism," cannot be correctly applied to these solar bodies and also to living beings. From any known living beings these bodies differ in structure, in composition, in size and form, and in action; for it will be shown that, in every kind of living matter known to man, certain actions take place which cannot possibly occur in the sun.

1. As to structure. Every living organism around us, and every elementary part of an organism, consists of matter in two very different states. It is not possible to find a piece of any living tissue as much as $\frac{1}{500}$th of an English inch in diameter which exhibits uniformity of structure throughout.

In answer to this asserted difference in structure between living tissues and inorganic matter, it will be said—Corresponding differences in structure *are* observed; for example, the section of a plum-pudding stone, of granite, of certain homogeneous viscid substances subjected to the action of a voltaic current, &c., shows masses or spaces more or less isolated, exhibiting certain characters, in the substance

of a matrix possessing characters and properties very different. Stones and brick embedded in mortar are like the "cells" embedded in the "intercellular substance" of which such a tissue as cartilage is composed. To this I answer as follows:—

a. Active and constant changes take place in the small collections of matter of which the so-called "cells" of the living tissue are partly composed, while the stones embedded in the mortar undergo no change.

b. The intercellular substance of the living tissue is *not deposited* around the "cells" like mortar around the stones, but it is formed from them. All the matter forming the "intercellular substance" or "cell wall" was at an earlier period in the same state as that matter of which the so-called "cells" are constituted. Now the mortar can exist without the stones embedded in it, but no "intercellular substance" was ever formed without "cells." And mortar may be made first, and then stones may be embedded in it; but "intercellular substance" is never formed first, and "cells" forced into it, or caused to appear in it. Stones can never produce the mortar which surrounds them, but "cells" always exist before the "intercellular substance," and always take part in its production.

c. The soft granular matter of which the "cell" is composed during the living state gradually passes by continuity of structure into the surrounding material—termed "intercellular substance" in some tissues, "cell wall" in others.

d. The soft granular matter of which the "cells" are composed exhibits differences of appearance in different parts. And I shall adduce evidence to show that growth in these masses occurs from the centre. These zones have received arbitrary names, as "Nucleolus," "Nucleus,"—but *names* are unimportant to the argument. No such differences, it need scarcely be said, exist in the stones. When the latter are embedded in the mortar no further change occurs; but it is easy to show that most active and important changes occur in the "cells," although they are surrounded by "intercellular substance."

e. The smallest particle of each little mass or cell of a living tissue *selects, forms, converts,* and can communicate these wonderful powers to inanimate matter which comes into contact with it, but the surrounding "intercellular substance" or "cell wall," does not possess these wonderful powers which are peculiar and correctly termed VITAL powers. Hence no true analogy can be drawn between "cells" embedded in "intercellular substance" and bricks or stones embedded in mortar, until the bricks or stones make themselves out of matter differing from them and from the mortar in composition, multiply in number of their own accord, and form and deposit the mortar which surrounds them.

Before the bodies in the photosphere of the sun can be compared to organisms, it must be shown that they possess some sort of *structure,* or at least it should appear reasonable to assume that some such difference in structure as can be shown to exist in all living tissues without exception, was possible and probable. But all known organisms differ from these bodies and from every kind of inorganic matter in many particulars besides structure.—L. S. B., King's College, London, in *The Reader.*

Massachusetts Dental Association.—The first annual meeting of the Massachusetts Dental Association was held on Tuesday, the 16th inst., at which the following named gentlemen were chosen officers for the ensuing year:—N. C. Keep, M.D., *President.* I. J. Wetherbee, D.D.S., *Vice President.* T. H. Chandler, *Recording Secretary.* E. C. Rolfe, M.D., *Corresponding Secretary.* S. J. McDougall, *Treasurer.* E. N. Harris, D.D.S., *Librarian.* I. A. Salmon, A. A. Cook, B. S. Codman, M.D., H. F. Bishop, D.D.S., T. H. Hitchcock, M.D., *Executive Committee.* Dr. N. C. Keep was also appointed to deliver the address at the next annual meeting; Dr. I. J. Wetherbee as substitute.

Thomas H. Chandler, *Secretary.*

American Medical Association.—That the organization of the meeting may be effected with as little delay as possible, and at the appointed hour of 10, A.M., the Committee of Arrangements particularly request that the secretaries of the several bodies purposing to send delegates to the Association, would forward their appointments *immediately* upon their being made. This will be the *voucher* for the delegate, and will avoid every embarrassment in registration. The delegate may present his voucher from the secretary. Delegates desiring to read "papers, essays, or memoirs," will send early the title and time required to read them. The Committee will be in session on Monday, 6th, from 12 o'clock to 5, P.M., and Tuesday morning at 8, in the Irving Hall, Irving Place.

James Anderson,
Chairman, 30 University Place, New York.

The Illinois State Medical Society held its 12th annual session in Chicago, commencing on the 12th inst., and continuing three days. Among the papers read were two on cerebro-spinal meningitis— by Dr. J. Adams Allen and Dr. R. E. McVey. Dr. J. S. Whitmore also reported verbally three cases of the same disease in his practice. A resolution was adopted to the effect that the present pay and rank of surgeons and assistant surgeons in the army are disproportionate to the services performed, and that proper exertions should be used for relief by national legislation on the subject.

VITAL STATISTICS OF BOSTON.

For the Week ending Saturday, May 21st, 1864.

DEATHS.

	Males.	*Females.*	*Total.*
Deaths during the week	48	41	89
Ave. mortality of corresponding weeks for ten years, 1853—1863,	36.1	33.7	69.8
Average corrected to increased population	00	00	76.85
Death of persons above 90	0	0	0

Deaths in Boston for the week ending Saturday noon, May 21st, 89. Males, 48—Females, 41.—Abscess, 2—accident, 3—apoplexy, 1—inflammation of the bowels, 1—disease of the brain, 1—inflammation of the brain, 1—bronchitis, 1—consumption, 12—convulsions, 2—croup, 7—dropsy, 5—dropsy of the brain, 2—drowned, 1—erysipelas, 1—scarlet fever, 5—typhoid fever, 2—disease of the heart, 2—insanity, 1—disease of the kidneys, 3—congestion of the lungs, 1—inflammation of the lungs, 11—marasmus, 1—measles, 2—old age, 2—scrofula, 1—smallpox, 6—disease of the spine, 1—teething, 2—unknown, 7—uterine disease, 1—whooping cough, 1.

Under 5 years of age, 41—between 5 and 20 years, 7—between 20 and 40 years, 15—between 40 and 60 years, 18—above 60 years, 8. Born in the United States, 70—Ireland, 14—other places, 5.